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ABSTRACT

Some criticisms leveled by Richard Bates and Thomas Greenfield against traditional views of the science of administration may be justified, but they do not apply to a science of administration based in the newly emerging philosophy of science. This paper begins by introducing these criticisms, which focus on the traditional views' failure to accommodate values, the subjective basis of knowledge, and the functional purposes of administrative theory. The paper next reviews the new philosophy of science, which incorporates new concepts about statements, evidence, terms, and the inexclusivity of theories. The new philosophy also involves trends away from foundational theories of justification and toward coherence theories of justification, as well as away from instrumentalism and toward realism. The implications of these factors for the development of a new science of administration are explored next. The importance of the role of human interpretation in understanding administration is examined in the paper's concluding section. Distinctions are drawn between physical events in which brute facts are measurable and social situations in which causal factors are exceedingly complex but can reasonably be assumed to be regular and structured. It is noted further that the notion of interpretation presupposes a reality subject to interpretation. Six pages of notes are provided. (PGD)

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Educational Administration and the New Philosophy of Science

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Paper presented at the Annual Meeting of the Philosophy of Education Society of Australasia (Armidale, New South Wales, Australia, September 1-7, 1986).

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Educational Administration and the New Philosophy of Science[I]

Abstract

In this paper, which was presented at the joint annual conferences of the Philosophy of Education Society of Australasia and the Group for Research in Educational Administration and Theory held at the University of New England, Armidale, in September I986, the author examines, from the perspective of the new philosophy of science, some of the arguments of two important critics of traditional views of science of administration; notably the arguments of Richard Bates and Thomas Greenfield. The author concludes that the new emerging views of science can sustain a science of administration that escapes their major criticisms.

A number of years ago Wayne Hoy, in lamenting the influence of practitioner oriented criteria of relevance and utility on the direction of research in educational administration, offered a defense of scientific research and theory development: "educational administration, as a discipline, can best preserve its own uniqueness by reaffirming its commitment to scientific research"[2]. Although Hoy's paper offered no systematic account of scientific research, he did suggest that it involved (I) "the systematic and empirical investigation of hypothetical propositions"[3] and (2) "an assumption that the nature of reality is ultimately material and knowable"[4].

Subsequent developments in the field indicate that the main threat to Hoy's conception of scientific research in educational administration is coming, not from the practical skeptic who questions the relevance and short term pay-off from this kind of research, but from the theoretical skeptic who questions the very possibility of a science of administration. Thus Richard Bates observes that Hoy's traditional view of science "is now regarded by a growing body of critics as a dubious and misleading (indeed, an ideological) view of how science is done"[5]. According to Bates "the theories of science are essentially constructed by scientists"[6], the process of science being

> a process of negotiation between competing claims influenced in its assumptions by social and political factors and subjected to constant amendment and change.[7]

Ironically, Bates cites as evidence for this new view of science, the work of a number of philosophers of science, notably Kuhn, Feyerabend, and Toulmin.[8] The point of citing these particular philosophers is not that they are agreed on the details of what constitutes this new view of science. They are not.[9] The point, rather is that they would apparently agree that as it is actually described traditional science is unscientific (according to the canons of the new view). What is wrong with the traditional view, according to Bates, is that it

> is based upon the idea of an independent reality which is subject to measurement and description, and explicable in terms of theory which

> can be verified by independent and impartial testing, resulting in propositions and explanations that are free from cultural and historical bias.[10]

Another theoretical skeptic who has developed systematic objections to a science of administration is Thomas Greenfield. Like Bates, Greenfield challenges the reasonableness of the traditional view of science. For Greenfield, science is not only something essentially constructed by scientists, it is irreducibly subjective. All scientists must face the fact that what passes for objective truth is based on what "can be perceived only subjectively".[11] On this view "truth is what scientists agree on or what the right scientists agree on".[12] Reality does indeed exist, but it can never be perceived directly; it is "always glossed over with human interpretations".[13] The main philosophical sources Greenfield cites in support of this view (apart from numerous general references to what he calls the Kantian tradition of epistemology) are, once again, the writings of Kuhn and Feyerabend, although he appears to equivocate on the question of whether a new (or perhaps broader) view of science could be used to underwrite a satisfactory science of administration.[14] In any event, his account of the essentials of social science (and hence administration) is clear enough:

> the purpose of social science is to understand social reality as different people see it and to demonstrate how their views shape the action which they take within that reality... the social sciences... must work directly with man's definitions of reality and with the rules he devises for coping with it. While the social sciences do not reveal ultimate truth, they do help us to make sense of our world.[15]

In this paper I propose to explore one central issue, developed in a number of different ways by theoretical skeptics of a science of administration, namely: the nature of the contribution that can be made to administrative theory by recent developments in the philosophy of science and the emerging new views of science. Traditional science of administration is increasingly thought to contain fundamental weaknesses. Examples from the work of Bates and Greenfield suggest: an inability to accommodate values in administrative theory, a failure

to recognise the provisional nature of all knowledge and its source in human subjectivity, and a major role in administrative theory's being a form of social control. My argument, which will focus largely on discussions by Bates and Greenfield, will be that while some of these criticisms of traditional administrative science in education may be justified (if that is indeed an accurate version of current administrative theory and practice) none carry over in any clear or obvious way to a science of administration informed by certain new views of science.

The New Philosophy of Science

In view of the importance being attached to recent work in the philosophy of science, it may be useful to specify, at least in general terms, some of its main features.[16] Of course, not all theorists would subscribe to all of the elements of this characterisation but, matters of emphasis aside, there is a fair measure of agreement over most of the following items.

I. There is no fundamental epistemological distinction between observation statements and theoretical statements. Rather, the statements of a scientific theory form a network.[17] Statements central to the structure of the network and the organization of inference, and apparently most removed from sensory experience - for example, laws of logic and mathematics, very general laws of physics like the first and second laws of thermodynamics - appear as the most theoretical part of the network, or at its 'centre'. Those statements that are, for example, singular reports of sensory experience, although formulated in ways that draw on some theoretical vocabulary, appear as the most observational part of the network, or at the 'periphery'.

2. Falsifying evidence is mediated by the network as a whole. Thus theory-laden observation reports may be discounted because they presume an inadequate theory. (Newton used his theory of optics to correct Flamsteed's 'falsifying observational data.) Network considerations also decide which part of the network, or even which statement, needs to be rejected where falsifying empirical evidence is accepted as true. In general, however, (to paraphrase Lakatos) you cannot get a direct empirical hit on a narrowly specified theoretical target.[18] For what has

gone into specifying the target is the whole network.

3. Confirming evidence always underdetermines a network or scientific theory. That is, the same finite body of empirical evidence can support a variety of systematically different theories. (Care needs to be taken in applying this thesis however, since as Quine[19] notes, there is no easy way to distinguish genuinely different theories from theories that are empirically equivalent but notationally different - for example where you use the word 'proton' for my 'electron' and vice versa.)

4. The meaning of a term is determined, in part, by its place in the network of statements comprising the theory.[20] (Where meaning is held to be entirely a matter of conceptual role, systematically different theories are claimed to be orthogonal or incommensurable, there being not enough equivalent structure to guarantee the univocality of any theoretical terms.)

The matter of what general methodological consequences flow from these features of the new philosophy of science is controversial. However, despite some important exceptions, two broad trends seem to be emerging. First, there has been a shift away from foundational theories of justification. That is, the practice of justifying knowledge claims by showing they can be derived, in some sense, from members of another class of more epistemically secure or knowable claims, is seen increasingly to be untenable, largely as a result of the collapse of the observation/theory distinction. Once epistemologists in the foundational tradition are seen to be using apparently less secure theory to identify the class of epistemically secure foundations - usually observations or first person sensory reports - the point of the procedure lapses. There is no neat correspondence between evidence and knowledge claims. This worry with foundational justification and its attendant correspondence view of evidence, has occasioned a shift towards coherence theories of evidence or justification.[21] Although a fuller version of this mode of justification will be displayed later, in the discussion of examples, we can note briefly that it has the following form: claims are justified if their addition to a network of claims enhances

the network's systematic virtues of consistency, coherence, comprehensiveness, simplicity, and fecundity.[22] Similarly, networks are to be judged against other networks on grounds of systematic virtue.

The second broad trend is a shift away from instrumentalism and towards realism. Detailed argument for this conclusion is complex, but in outline the strategy is simple enough. If we take, as uncontroversial touchstone or paradigms of the real, humdrum and familiar physical objects like chairs and tables, walls and doors, and if there is no sharp distinction between observation and theory, then there is no obvious methodological ground for regarding theoretical objects like atoms, electrons, or organizations as instrumental fictions rather than as real objects on the same ontological footing as their more familiar (though theory-laden) counterparts.[23] Of course, it may be argued within a theory network that chairs and tables are unreal, but in being denied access to any evidential foundations for such a claim, the implied alternative network standard of reality must cohere with what the network will admit as evidence for its standard. My suggestion concerning Greenfield's work on organizations is that his standard of reality, his conditions of objectivity, outrun whatever evidence they permit to be marshalled in their favour.

It is worth emphasising an important methodological point that is involved in conjoining these two trends. Being able to hold both a non-foundational view of justification on the one hand, and a realist perspective on the other, implies that one must distinguish a coherence theory of evidence from a correspondence theory of truth.[24] The former does the work in deciding a claim or theory network's merits over and above its rivals. But in deciding on the network's ontology, on what objects exist and how they correspond to words and sentences, we use as a guide the internal structure of the favoured network. In other words, it is the whole theory that tells us what exists, and how our language latches onto the world. Note the contrast with some traditional views of science where matters are around the other way; namely, favoured correspondences between claims and reality function as evidence for theories.

Finally, it should also be noted that the kind of internal network structure favoured by many realist philosophers of science, including Popper, Quine, and Hesse, is that imposed in meeting the requirements of Tarski's theory of truth.[25]

Given this account of the new view of science, what are its consequences for scientific objectivity in educational administration? Clearly, there are a number of important issues and objections to be addressed.

Science of Administration and the Management of Knowledge

Some of these objections, we have already seen, have been developed in the work of Richard Bates although, strictly speaking, this aspect of Bates's work has been about traditional conceptions of science. The interesting question therefore, is the extent to which they carry over to the new view. In this discussion I shall confine myself to three criticisms that he proffers, namely: that a science of administration is essentially manipulative and concerned with control; that the value of human betterment is an important basis for assessing knowledge claims in addition to scientific truthfulness; and that science, when seen as something essentially constructed by scientists, falsifies Hoy's assumption that the nature of reality is ultimately knowable.

Among other influences, Bates's understanding of science draws on a reading of the earlier work of Habermas, particularly the epistemological theses of Knowledge and Human Interests. In this earlier work, Habermas identifies

> three categories of processes of inquiry for which a specific connection between logical-methodological rules and knowledge-constitutive interests can be demonstrated....The approach of the empirical-analytic sciences incorporates a technical cognitive interest; that of the historical-hermeneutic sciences incorporates a practical one; and the approach of critically oriented sciences incorporates [an] emancipatory cognitive interest...[26]

Traditional (so called positivistic) science which Bates, following Habermas, identifies with empirical-analytic science, is seen as hypothetico-deductive after the Nagel/Hempel deductive nomological model, with predictive success a

measure of technical exploitability. Thus for Habermas

> theories of the empirical sciences disclose reality subject to the constitutive interest in the possible securing and expansion, through information, of feedback-monitored action. This is the cognitive interest in technical control over objectified processes.[27]

In Bates's hands, the technical scientific definitions of knowledge and rationality are argued to be far too narrow for social science, and need to be supplemented by critical discourse. A suitable broadening, he suggests,

> is argued at length by Habermas, who contends that the annexation of rationality by dominant scientific, technical, manipulative interests has prevented the continuation of an historical discourse directed toward a rational administration of the world...[28]

He goes further, asserting that "as currently conceived by professional and professor alike, educational administration is a technology of control".[29] And finally, in summarizing a robust and systematic sheeting home of the blame to poor philosophy, he declares:

> The inadequacies of the hypothetico-deductive model of positivist science and the positivist, apolitical model of society were argued to be intellectual products that provided the illusions necessary for the continued employment of techniques of hierarchical administrative control that perpetuate the injustices of an unequal society.[30]

There are a number of things that are puzzling about this account of science and administration. For example, if the traditional view of science is wrong, and we know that it is thanks to the work of Quine, Kuhn, Feyerabend, Hesse, and others, then the story Habermas tells of empirical science being <u>constituted</u> by technical interests of control and manipulation is also threatened. For Habermas's account of empirical-analytic science is as much dependent on traditional empiricist theories of science as the traditional science of administration that Bates is using Habermas's machinery to criticise. In more recent work, Habermas recognises some of these difficulties. Responding to Hesse's invokation of the

new philosophy of science in an evaluation of his work[31], he remarks:

> During the past decade I have been only cursorily occupied with questions in the theory of science and have lost the feeling for the relevant discussions....In the light of the debate set off by Kuhn and Feyerabend, I see that I did in fact place too much confidence in the empiricist theory of science in Knowledge and Human Interests.[32]

Habermas is right to be concerned about his earlier prominent use of this model. The powerful coherentist-realist theories of scientific knowledge inspired by Quine's writings and developing out of research conducted in the I970s by philosophers like Hesse, Hooker[33], and Churchland[34], make the task of arguing a constitutive theory of science based on instrumentalistic technical control and manipulation, a very large task indeed.

Another puzzle concerns the status of traditional science of administration as a necessary illusion for promoting control. The obvious problem with the old philosophies of science was that the practice of science and the growth of scientific knowledge did not occur in the ways required. Why should the transplanted administrative version, perhaps held by professional and professor alike, be any more accurate? Perhaps the practice of administration and the growth of administrative knowledge is systematically misdescribed by traditional theories. Given his intellectual apparatus, it is difficult to know how Bates can settle this issue without begging the question. For example, he cites some of Kerr's arguments[35] for the existence of systematic relations between particular conceptions of knowledge and particular institutional structures. But the analysis of administrative structures in terms of his required technical control presumes the adequacy and applicability of the questionable Habermasian analysis of empirical science.

Perhaps what he means is that if professionals and professors adopt a traditional science view of administration then hierarchical administrative control that perpetuates injustice will result, or where it exists, be reinforced. Now this claim includes an interesting and substantive empirical thesis concerning a particular pattern

of social causation, and one moreover that is not known to be true (Kerr's work notwithstanding). It would, of course, be much easier to argue this kind of case if we could identify one major, well analysed body of theory and practice that instantiated the conditional. But the obvious example, namely the theory and practice of empirical science, is now known to be disanalogous in crucial ways. Nor is it clear that the larger theory of social causation likely to be invoked to sustain the inference, will itself be free from errors associated with traditional views of science.

A final worry over this issue of science, manipulation, and control needs to be addressed. From the point of view of production of scientific knowledge, one can, perhaps, see how an emphasis on manipulation and control might arise; regularities appear to be sifted and discerned by simplifying different aspects of a complex environment through manipulation of variables. Mill's methods, the prescriptions of Nagel, and the recipes in most textbooks on quantitative social research, give the details with varying degrees of plausibility.[36] However, from the perspective of learning theory, it is difficult to see how most learning could occur without some kind of active manipulative intervention by the learner in an environment. Ironically, the theorists who come closest to denying this point are the classical empiricists; for Locke, Hume, and even some of the Vienna Circle Positivists, the business of receiving sensory impressions can be an entirely passive affair. One of the advantages of the new coherence pattern of justification is that in denying any a priori, or first philosophy, and in treating epistemology naturalistically as a scientific problem of knowledge acquisition, it is able to draw on the most sophisticated theories of (individual and collective) learning to help account for the growth and development of science itself. The complexity, structure, and sheer range of resources available to this methodology suggests that it not only undermines some of the content of Bates's critique of administrative control, but also much of the theoretical machinery he uses as well.

One of the problems that traditional empiricist theories of knowledge shared

was the problem of giving an adequate account of value statements. Sensory evidence, when mapped in packages one-to-one onto individual statements, seemed to give grounds for maintaining factual claims, but appeared to have nothing to do with value claims. Thus, where a bundle of observation reports or sensory experiences might give reason for believing that, for example, resources had been distributed in such and such a way, there seemed to be no further or additional sensory evidence that corresponded obviously to the claim that the distribution was unjust. Facts were one thing, values another. On this view factual claims could be true because they corresponded to a definite range of sensory experience, but the notion of truth was inapplicable to value statements. At most it was true that people had preferences, and so critics of traditional empiricist theories of value objected (and still do) to the collapse of value into apparently mere preference.[37]

Now one of the doctrines that fails to find support in the new philosophy of science is the belief in a sharp separation of fact and value. Consequently, it is surprising to find Bates arguing for a view "in which ideas of truth and validity are supplemented by critical discourse over the ways in which knowledge serves the course of human betterment".[38] It is as though he is accepting an old positivist notion of truth as correspondence with empirical evidence. But it is precisely correspondence evidence that has been dealt the heaviest blows in recent times; attacks on the fact/theory distinction, naive falsificationism, classical confirmation theory, and the defense of conceptual role theories of meaning - in short, the key features of the new philosophy of science - all are supported by the attack on correspondence evidence, on the belief that to each statement there corresponds a unique range of sensory, or observational, evidence.

The proper response here is not to separate value from truth, but to incorporate value statements into the whole network of statements that face the tribunal of sensory evidence as a corporate body.[39] The defense of a value claim, like the defense of any theoretical claim, is thus conducted by showing how it is part of a theory network that is more coherent than alternative

rival networks. This is, of course, a piecemeal and tentative business but it is one that does not, for mistaken epistemological reasons, quarantine statements from participation. If we reserve the word 'true'for statements making up the network we judge to be best after the theory of evidence has done its work, then 'true' may apply univocally to all the statements of that network. Given a holistic theory of evidence, a felicitous parsimony obtains in being able to say that "'such and such a programme makes for human betterment' is true". In wanting to supplement truth with human betterment, Bates is possibly being misled once again by Habermas's crude view of science.

Earlier, I speculated that traditional science of administration may systematically misdescribe administrative practices. I think that when it comes to the matter of values this charge is correct. Writers such as Bates, Hodgkinson[40] and Greenfield[41] have persistently drawn attention to the fact that administration involves value issues at all levels of theory and practice. Indeed, they have used precisely this point to reject as inadequate a science of administration. What is often not realised however, is that the new view of science contains the resources for overcoming this objection. Those who reject traditional science still have to confront the question of how to adjudicate competing value claims. But if correspondence evidence is believed to be both inadequate for values and also <u>all the evidence there is</u>, then choosing among competing value claims is apt to become something that is conducted in an empirical vacuum. The upshot is decision procedures like Hodgkinson's appeal to 'trans-rationality' or Greenfield's 'anarchy'. The new view, on the other hand, acknowledges that the significance of a term like 'human betterment' is due partly to the role it plays in an elaborated theoretical structure that, as a whole, has extensive empirical consequences. Where the network is revised in devious but, hopefully systematically virtuous ways to accommodate what are, by its own admission, unexpected consequences, the pressure for revision falls as much on value claims as on any other set of theoretical claims in the network. This holistic account of the theoretical unity of fact and value thus has the potential to take into its

purview such humdrum administrative phenomena as the altering of very broad (moral) goals in the light of theories of what is achievable, and administrator moral development through learning from experience.

The final aspect of Bates's theory to be considered here is his denial of the assumption that reality is ultimately knowable, a position, we saw, that figured centrally in his attack on Hoy. Since this claim admits of a variety of interpretations I shall, in the main, be concerned with attempting to discern and evaluate, in the light of the new philosophy of science, what non-trivial doctrines are being denied.

One possibility is that he is denying there is any theory-free way of describing reality. Perhaps, although by itself that is hardly an attack on the belief that reality is ultimately knowable. Any words we use to describe the world will be drawn from some theory network. True, some words, like 'chair' and 'table' will seem more basic than others, like 'electron' or 'organization', but in scrutinizing the contents of sensation - "variformed and varicoloured visual patches, varitextured and varitemperatured tactual feels, and an assortment of tones, tastes, smells, and other odds and ends"[42] - chairs and tables are no more to be found there than electrons and organizations. Our positing of chairs and tables (or electrons and organizations for that matter) is reasonable because these are just the sorts of objects our most evidentially superior theory supports.

Perhaps it is the ultimateness of these posits that is being challenged. Here we can agree, noting that it has been commonplace, since Popper first began making the point, to regard all our scientific knowledge as provisional.[43] We can never rule out the possibility of current theories being overthrown in favour of better ones. We can do no better than work within the bounds of our current best theories.

Another, more controversial, suggestion is that Bates is denying that some theories are better than others. In citing Kuhn and Feyerabend he may have in mind their thesis that some large scale scientific theories are incommensurable:

literally incomparable, to the point where it makes no sense to say that one is better or worse than another. This doctrine enjoys some currency among sociologists where it is used to underwrite a species of epistemological relativism, to the effect that one view is presumed to be as reasonable as any other. Its ontological equivalent states that different realities are constructed or fabricated by scientists, but that there is no basis for preferring one 'world' or reality to another. One merely inhabits rather than rationally prefers these worlds. Greenfield's writings carry more than a suggestion of this doctrine.[44] Moreover, in drawing on the writings of Kuhn and Feyerabend, it appears to issue from the new philosophy of science, especially with its emphasis on meaning as conceptual role within a network.

If there is a mere trace of this view lying behind the sense in which Bates sees scientists as constructors of theories, then it will have problems. The central one from which others issue concerns the fact that this view of incommensurability draws on a highly implausible version of the conceptual role theory of meaning. Earlier, I suggested that one feature of the new philosophy of science was its view of meaning as partly determined by conceptual role. However, in the writings of Kuhn[45] and Feyerabend,[46] what is needed to sustain incommensurability is the belief that meaning is entirely determined by conceptual role. If this thesis is accepted then not only do incommensurable theories become in principle unlearnable, but we cannot learn even our own theory. This conclusion turns on the fact that in order to understand a small part of a theory, say a handful of words, we would need to understand the whole theory in order to appreciate the conceptual role played by these words. Here the theory of meaning begins to outrun its posited resources: it posits learned antecedents of learning that cannot themselves be learned.[47] After the debates with traditional philosophy of science initiated in the I960s by Kuhn and Feyerabend, one of the doctrines to go in the newer emerging coherentist-realist views from the I970s, was the doctrine that anything goes.

Interpretation in Educational Administration

For Thomas Greenfield traditional science of administration is mistaken, particularly in its pretensions to objectivity, because it ignores the fundamental and irreducible contribution of human interpretation, and hence human subjectivity. According to Greenfield, interpretation enters administrative theory in two main ways. The first concerns the realm of knowing covered by traditional science. In his epistemology he follows Kant in drawing

> the distinction Kant drew between the noumenal world (the world as it is) and the phenomenal world (the world as we see it). For Kant, a world of reality does indeed exist but man can never perceive it directly; reality is always glossed over with human interpretations... In popular form, the Kantian philosophy has been expressed as follows: 'man does not create his world but he does make it'.[48]

Perhaps the only realities 'out there' that are not subject to our control, Greenfield speculates, are birth and death.[49] For the rest it is a matter of human interpretation, a matter (to some extent) of how we choose to see the world. As the discussion in the preceding section showed, however, this kind of subjectivism has its theoretical limits; we cannot make just any world of our choosing. It also has very practical limits. For example (as I have argued elsewhere):

> When Greenfield returns to his office he presumably comes back through the door in the wall rather than through the wall. Of course his assumption that a solid wall is not as deformable on bodily contact as an air space is an interpretation of experience. It's just that some interpretations are better than others. But how can one interpretation be better than another if there is no direct matching up of interpretation with reality, if reality is by and large unknowable? The fact that no matching up can be achieved even in principle suggests that something has gone wrong with Greenfield's standard of reality. What he has presented is a successful reductio argument against his

> own formulated requirements. Success in getting in and out of rooms is about as basic as objectivity gets in this context.[50]

It is indeed about as basic as objectivity gets in _any_ context. The fact that evidence fails to satisfy the 'direct perception' requirement does not imply that we have no workable or adequate alternative to epistemological anarchy. If the new philosophy of science is sound, we know that evidence has _never_ satisfied a 'direct perception' requirement; and yet science has developed powerful explanatory and predictive theories concerning a wide range of phenomena. It is therefore not clear why the presence of interpretation in this first sense should prevent a viable and comprehensive science of administration.

The second way in which interpretation enters administrative theory is the more fundamental for Greenfield's characteristic view of organizations as socially constructed reality. We may, perhaps, concede that science can deal with _brute data_, with chairs and tables, and "facts that press themselves upon our understanding so forcibly that no one can question their existence"[51], but organizational reality is defined by non-brute data, by meanings, human intentions, actions, and experience. On this view "organizations are... cultural artifacts: they are systems of meaning that can be understood only through the interpretation of meaning".[52] Some examples might illustrate this contrast. In describing the moves in a game of chess, science is supposed to be limited to descriptions like: such and such a shaped piece of matter was moved from one particular position on a checkered board to another position, whereupon all the shaped pieces were swept from the board and placed in a box. With the resources of interpretation and an understanding of the rules of chess, on the other hand, we may say that the white rook checkmated an opponent's king. In this case it seems that we cannot understand (or even predict) what is happening without a grasp of the relevant rules that _constitute_ or define the practice of playing chess, of winning and losing. Similarly, in organizational theory, to adapt an example from Greenfield,[53] we may regard as a brute datum the fact that a person died as a result of a fall from a tall building, but whether the person

accidentally fell or was murdered, whether the person was a criminal or a hero, are matters far removed from brute data. These are social facts that draw on interpretations and interpretations of interpretations. For Greenfield "the important facts of organizational life call non-brute data into play... There is therefore no ultimate reality in the understanding of organizations".[54]

What Greenfield seems to be suggesting is this: whatever the facts are, and however reliable our knowledge of them may be, when it comes to the matter of interpretation, there is no fact of the matter, no ultimate reality to be right or wrong about, no ultimate social truth. For example, there is nothing in the physical distribution of matter bounding a game of chess in progress that is a checkmate. A checkmate is the interpretation we choose to give a certain physical configuration; it is something over and above the physical reality described by science. No scientific study of the physical players-pieces-board configuration will evidently enable us to predict the conditions under which the game terminates unless we supplement the study with an account of the rules of chess, what the various moves mean, and how we interpret the various pieces.

And so it is with organizations. It is a mistake to regard them as just people-environment physical configurations that interact according to sets of causal laws. Rather, they are created and sustained by the intentional, meaningful, rule governed action of people. Social non-brute facts that are the stuff of organizations, like signing a cheque, bidding at an auction, or issuing an order, cannot be understood except in terms of comprehensive networks of rule governed, intentional actions. Issuing orders, or bidding, as non-brute facts, bring about the consequences they do because other people interpret these actions the way they do. If people systematically interpreted these actions differently, the physical configurations accessible and describable by science, would cease to bring about the consequences they do. Indeed, they would cease to be the non-brute facts they are. Social reality itself would have altered because "it is non-brute facts themselves that define

and constitute social reality".[55] Thus science, traditional or otherwise, inasmuch as it uses a causal/descriptive explanatory apparatus that abstracts from interpretation, cannot in principle account for the kind of social and organizational phenomena that form the locus of administrative theory.

While perhaps effective against certain expressions of traditional science of administration, this whole argument of Greenfield's has quite definite limitations, as will become evident when it is pitted against some of the constraints imposed by the newer views of science. Although the issues here are complex, the key point on which I want to focus can be expressed quite simply. Either the truth conditions for interpretations are epistemically accessible or they are not. If not, then we have a closed and vicious hermeneutical circle which gives us no reason for supposing that interpretations can explain anything at all. If they are, then interpretations are best viewed, despite their intentional idiom, as networks of hypotheses after the fashion of scientific theories.

A writer cited by Greenfield in support of a hermeneutical science is Charles Taylor, who argues that

> if we have a science which has no brute data, which relies on readings, then it cannot but move in a hermeneutical circle. A given reading of the intersubjective meanings of a society, or of given institutions or practices, may seem well founded, because it makes sense of these practices or the development of that society. But the conviction that it does make sense of this history itself is founded on further related readings... But if these readings seem implausible, or even more, if they are not understood by our interlocutor, there is no verification procedure we can fall back on. We can only continue to offer interpretations; we are in an interpretative circle... a hermeneutic science cannot but rely on insight.[56]

Not all researchers in educational administration who have been attracted to hermeneutical science have been happy with these conclusions. Thomas Sergiovanni,

who also cites Taylor on social reality and non-brute facts, sees hermeneutics as the lesser of two evils:

> it is far better to adopt the concept of theory of practice and the hermeneutical mode of inquiry, no matter how difficult this task, than to continue with the more comfortable but less appropriate methods and images of traditional science.[57]

Given this limited set of options he feels "left with no other alternative but to proceed in these murky waters".[58] Greenfield also expresses some concern:

> The task of choosing images, metaphors, statements to represent the organization as a cultural artifact is not easy. There are no rules for such a representation, for the explication and interpretation of reality.[59]

Both Sergiovanni and Greenfield are right in seeing a problem here, for if there are literally no rules - explicit, implicit,tacit,or otherwise - the task does not become difficult, or even impossible; the task ceases to exist. If there is no (presumed) standard or measure of suitability of choice among images and metaphors, and therefore no mark to fall short of, the notion of representation becomes idle. There would literally be no such thing as a success or a failure to represent. This point is apt to be disguised by talk of 'intuition', 'illumination', 'insight', or even the 'naturalness' of interpretations. But make no mistake: whatever it is that distinguishes non-insightful from insightful interpretations, it is not to be found in the distinction between fiction and non-fiction. Not unless there is more to representation than the methodology of the hermeneutical circle can concede. If administrative theory is to be pursued as a non-fiction genre therefore, we must abandon the methodology that leads to this circle. (Greenfield appears to admit as much, at least by implication, when he comes to the practical task of giving an account of how administrators should be trained. In suggesting <u>something</u> rather than nothing at all - spending time in a monastery, or as a patient in a mental hospital, for example[60] - he is attempting what amounts to a

rough specification of some of the empirical conditions under which insight is more likely to be produced.)

Fortunately, the resources of the new philosophy of science provide some scope for developing an alternative to Sergiovanni's "murky waters". To see this consider again the example of the game of chess. Suppose we use our science to build and program two machines, A and B, to play each other chess. In watching this active configuration of A plus B plus display monitor, we may be moved to say that A has made a mistake, B has adopted a better strategy or is playing a better game, and that B will shortly checkmate A's king and win. And if we are sufficiently familiar with the ways of chess and correct in our assumption that the machines are playing rationally our comments will no doubt enjoy high predictive accuracy. But there is nothing non-physical or non-causal going on in the configuration that makes the descriptions 'mistake', 'adopted a better strategy', 'checkmate', or the prediction that B will win, particularly appropriate. Each move and counter-move has its causal antecedents, if we assume, not unreasonably, that the rules of chess and certain optimizing subroutines are "hard wired" into A and B as standing causal dispositions, and so the entire sequence proceeds according to the regularities of physical science. Winning, losing, making mistakes or deft moves, all have their fine-grained physical correlates in the A plus B plus monitor system, and scientific knowledge of the behaviour of the system at this level of description will yield predictions of wins and losses that are in principle as accurate as the system is deterministic.

The point of this example however, is not to suggest that a science of chess - or a science of organizations - consists in the giving of engineering details. At that level of detail the causal story as it would currently be told is too complex to be of any general use. The point worth emphasising is that such detail is, by and large, not needed in describing and accurately predicting either chess outcomes or a great many ordinary, everyday social or organizational outcomes. This is because our ordinary ways of describing complex systems like chess computers - winning and losing, making good and bad moves - already assume

an underlying regularity of causal structure. For example, if machine hardware was unreliable or arbitrary in its behaviour, we could not use our broad empirical assumption of machine rationality to predict chess outcomes. And so it is with vastly more complex causal structures like organizations. In criticising the possibility of a science of administration, Greenfield has failed to notice that ordinary language talk of intentions, reasons, beliefs, and desires - in short our whole apparatus of folk psychology[61] - assumes the regularity and reliability of vast amounts of causal structure in people and their environment. Greenfield, and a great many other writers in modern social theory,[62] still regard it as an acceptable commonplace to say that a person's behaviour is caused only when the person does not act rationally, or in accord with folk psychological assumptions.

Earlier, I suggested that the causal/descriptive explanatory apparatus of science was thought to be inadequate because it abstracts from human intentions, interpretations, desires, and the like. Now I want to suggest that a strength of explanation in terms of these folk notions is that it abstracts from the engineering details of complex systems. Actions our language effortlessly describes as writing a cheque, issuing an order, or bidding at an auction all presume such vast networks of causal regularities that we could at present hardly begin to describe them in engineering terms. Nevertheless, the autonomy of thought and action implied by ordinary, everyday language is an illusion, sustained mainly by the sheer ubiquity of the network of regularities around us. Greenfield's view of interpreted social reality is therefore not an alternative to a science of organization. The whole enterprise of interpretation can only proceed on the assumption that there is a reality there to be interpreted, and that it is sufficiently orderly and regular to sustain the kind of empirical abstractions typical of interpretations. For the hallmark of a good interpretation, on this view, is the extent to which its claims will mesh coherently with what the most scientific parts of our total theory have to say. Where complexity permits, the value of a new and systematic science of

administration will lie chiefly in the convergence of these two types of abstraction. Complexity notwithstanding, the ancient social realities of weather and climate succumbed long ago. One is at least entitled to locate present social realities on that same agenda.[63]

NOTES

I. An earlier version of this paper was presented to a meeting at Monash University, Victoria, Australia. For valuable comments I would like to thank Lawrie Angus, Jill Blackmore, Judith Chapman, Peter Gronn, Gabriele Lakomski, and Jim Walker.

2. W.K. Hoy, "Scientific Research in Educational Administration", Educational Administration Quarterly I4, 3 (I978): I.

3. Ibid.

4. Ibid., p.3.

5. R.J. Bates, "Educational Administration, Sociology of Knowledge, and the Management of Knowledge", Educational Administration Quarterly I6, 2 (I980): 5.

6. Ibid.

7. Ibid., p.6.

8. Bates, "Educational Administration", pp. 5-6. Bates also mentions Lakatos in his Educational Administration and the Management of Knowledge (Victoria: Deakin University Press, I983), where he gives a fuller account of the connections among the new philosophy of science, the new sociology of education, and his view of educational administration as the management of knowledge (and culture).

9. See, for example, I. Lakatos & A. Musgrave, eds., Criticism and the Growth of Knowledge (London: Cambridge University Press, I970), which contains contributions from Kuhn, Lakatos, Feyerabend, Toulmin, and also Popper.

I0. Bates, "Educational Administration", p.4.

II. T.B. Greenfield, "The Man Who Comes Back Through the Door in the Wall: Discovering Truth, Discovering Self, Discovering Organizations", Educational Administration Quarterly I6, 3 (I980): 92.

I2. T.B. Greenfield, "Reflections on Organizational Theory and the Truths of Irreconciliable Realities", Educational Administration Quarterly I4, 2 (I978): 8.

I3. T.B. Greenfield, "Theory about Organizations: A New Perspective and its Implications for Schools", in Administering Education: International Challenge, M.G. Hughes, ed. (London: Athlone Press, I975). Cited as reprinted in P.C. Gronn, Rethinking Educational Administration: T.B. Greenfield and his Critics (Victoria: Deakin University Press, I983), p.7I.

I4. See, for example, the discussion in the first few pages of T.B. Greenfield, "The Decline and Fall of Science in Educational Administration", Interchange I7, 2 (I986).

I5. Greenfield, "Theory about Organizations", p.72.

I6. For other examples of this see M. Hesse, "Duhem, Quine, and a New Empiricism", in Knowledge and Necessity, G. Vesey, ed. (London: Macmillan, I970), pp.I9I-209, and "In Defence of Objectivity", Proceeds of the British Academy 72 (I973): 275-292.

I7. For more on the network model see W.V. Quine & J.S. Ullian, The Web of Belief, 2nd ed. rev. (New York: Random House, I978), and M. Hesse, The Structure of Scientific Inference (London: Macmillan, I974).

I8. See I. Lakatos, "Falsification and the Methodology of Scientific Research Programmes", in Lakatos & Musgrave, Criticism and the Growth of Knowledge, pp. 9I- I96 (although I disagree with his treatment of the 'Duhem-Quine Thesis).

I9. W.V. Quine, "On Empirically Equivalent Systems of the World", Erkenntnis 9 (I975): 3I3-328.

20. The classic statement of this position is W.V. Quine, "Two Dogmas of Empiricism", Philosophical Review 60 (I95I): 20-43. Cited as reprinted in W.V. Quine, From a Logical Point of View (Cambridge Mass.: Harvard University Press, I96I), pp. 20-46. See also Hesse, Scientific Inference, pp. 9-73.

2I. A good introductory account of these trends is W.V. Quine, "The Nature of Natural Knowledge", in Mind and Language, S. Guttenplan, ed. (London: Oxford University Press, I975), pp. 67-8I.

22. A discussion of these virtues can be found in W.V. Quine, "Posits and Reality", in The Ways of Paradox and Other Essays, W.V. Quine, revised and enlarged edition (Cambridge, Mass.: Harvard University Press, I976), pp. 246-254. For a more detailed survey see C.W. Evers, "Epistemology and Justification: From Classical Foundationalism to Quinean Coherentism and Materialist Pragmatism", in Epistemology, Semantics, and Educational Theory, C.W. Evers & J.C. Walker, eds. (Department of Education: University of Sydney, I984), pp. I-29.

23. See W.V. Quine, "The Scope and Language of Science", in Quine, Ways of Paradox, pp. 228-245.

24. The clearest discussion of this distinction can be found in G. Harman, "Quine on Meaning and Existence II", Review of Metaphysics 2I, 2 (I967): 343-367.

25. There is a good introduction to Tarski in W.V. Quine, Philosophy of Logic (Englewood Cliffs: Prentice-Hall, I970), pp. 35-46.

26. J. Habermas, Knowledge and Human Interests (Boston: Beacon Press, I97I), p.308.

27. Ibid., p.309.

28. R.J. Bates. "New Developments in the New Sociology of Education", British Journal of Sociology of Education I, I (I980): 68.

29. Bates, Educational Administration and Management, p.46.

30. Ibid., p.30.

3I. M. Hesse, "Science and Objectivity", in Habermas: Critical Debates, J.B. Thompson & D. Held, eds. (London: Macmillan, I982), pp. 98-II5.

32. J. Habermas, "A Reply to My Critics", in Thompson & Held, Habermas: Critical Debates, p.274.

33. C.A. Hooker, "Systematic Realism", Synthese, 26 (I974): 409-497.

34. P.M. Churchland, Scientific Realism and the Plasticity of Mind, (London: Cambridge University Press, I979).

35. The paper he cites is D.H. Kerr, "Knowledge Utilization: Epistemological and Political Assumptions", Knowledge: Creation, Diffusion, Utilization 2, 4 (I98I): 483-50I.

36. A good introduction to philosophy of science from a more traditional perspective that exemplifies some of these points on manipulation of variables is C.G. Hempel, Philosophy of Natural Science (Englewood Cliffs: Prentice-Hall, I966).

37. I discuss some of these points in C.W. Evers, "Hodgkinson on Ethics and the Philosophy of Administration", Educational Administration Quarterly 2I, 4 (I985): 27-50.

38. Bates, "New Developments", p.68.

39. Quine, "Two Dogmas", pp.42-46.

40. C. Hodgkinson, Towards a Philosophy of Administration (Oxford: Basil Blackwell, I978).

4I. See, in particular, T.B. Greenfield, "Putting Meaning Back into Theory: The Search for Lost Values and the Disappeared Individual", a paper presented to the Annual Conference of the Canadian Society for the Study of Education, University of Montreal, 28 May I985.

42. Quine, "Posits and Reality", p.250.

43. There is a discussion of Popper's views in relation to those parts of Herbert Feigl's views that influenced educational administrators in the early days of the Theory Movement in C.W. Evers, "Philosophical Research in Educational Administration", Ways and Meanings of Research in Educational Administration, R.J.S. Macpherson, ed. (Armidale: University of New England Press, I986).

44. See especially T.B. Greenfield, "Against Group Mind: an Anarchistic Theory of Organizations", in Reflective Readings in Educational Administration, R.J. Bates, ed. (Victoria: Deakin University Press, I983).

45. For a summary of Kuhn's original position on this see T. Kuhn, The Structure of Scientific Revolutions, 2nd. ed. enlarged (Chicago: University of Chicago Press, I970), pp. I48-I59. Here he says that "the transfer of allegiance from paradigm to paradigm is a conversion experience that cannot be forced" (p.I52.). He says that scientists can be persuaded but that "paradigm

change cannot be justified by proof" (p.I52.). In his Postscript I969, Kuhn begins to retreat, denying that he ever said "there can be no recourse to good reasons" for a paradigm change (p.I99.). However, I think his original views on incommensurability stand, and that in the Postscript he equivocates over "good reasons" between reasons that persuade and reasons that justify.

46. P.K. Feyerabend, Against Method (London: Vesso,I975), pp. 223-285. For a reply, see M. Devitt, "Against Incommensurability", Australasian Journal of Philosophy 57, I (I979): 29-50.
47. This argument is developed in P.A. Roth, "Paradox and Indeterminacy", Journal of Philosophy, 75, 7 (I978): 347-367.
48. Greenfield, "Theory about Organizations", p.7I.
49. Greenfield, "Against Group Mind", p.294.
50. Evers, "Philosophical Research"
5I. T.B. Greenfield, "Leaders and Schools: Willfulness and Nonnatural Order in Organizations", in Leadership and Organizational Culture, T.J. Sergiovanni & J.E. Corbally, eds. (Chicago: University of Illinois Press, I984), p.I50.
52. Ibid.
53. Ibid., pp. I50-I5I.
54. Ibid., p.I5I.
55. Ibid.
56. C. Taylor, "Interpretation and the Sciences of Man", in Interpretive Social Science: A Reader, P. Rabinow & W.M. Sullivan, eds. (Berkeley: University of California Press, I979), pp. 65-66.
57. T.J. Sergiovanni, "Developing a Relevant Theory of Administration", in Sergiovanni & Corbally, Leadership, p.289.
58. Ibid., p.290.
59. Greenfield, "Leaders and Schools", p.I56.
60. Greenfield, "The Man Who Comes Back", p.I08.
6I. For a critical view of folk psychology see P.M. Churchland, "Eliminative Materialism and the Propositional Attitudes", Journal of Philosophy 78, 2

(I98I): 67-90.

62. The most recent influential writer to make this mistake throughout his work is Anthony Giddens.

63. For an important survey of some of the aspects of this theory relevant to sociology of education see J.C. Walker, "Materialist Pragmatism and Sociology of Education", British Journal of Sociology of Education 6, I (I985): 55-74.